shine was reported for the past winter compared to the average of a decade ago) as a result of restrictions on the use of "dirty" fuels shows that pollution produced by a very large population can be reduced by feasible measures. When there are dramatic instances of polluted air and water, or when wildfowl produce eggs with inadequate shells as a result of the ingestion of DDT, it does not automatically follow that the world is overpopulated. I have pointed out before that pollution of air and water is a serious problem in parts of Australia.

The authors seem to make no attempt to strike a balance in the evidence they examine. An illuminating instance is their discussion of the so-called "Green Revolution" on pages 96–101. This set of innovations (involving the use of new high-yield varieties of grain, combined with multiple cropping, increased use of fertilizer, and irrigation) is not, I am glad to say, treated with the scorn that has characterized some of its discussion by ecologists. Many of the encouraging developments (though by no means all) are described, but almost every favorable statement is immediately followed by an expression of doubt. For example:

> Probably the most widely recommended means of increasing agricultural yields is through more intensive use of fertilizers. Their production is straightforward, and a good deal is known about their effective application. But the environmental consequences of heavy fertilizer use are ill-understood and dangerous (Chapter 7). Even if we could ignore such problems, we find staggering difficulties barring the implementation of fertilizer technology on the scale required.

The section ends as follows:

> Even the most enthusiastic boosters of the Green Revolution admit that it cannot possibly keep food production abreast of population growth for more than two decades or so. Since a birth control solution to the population explosion will inevitably take longer than that, the prospects for avoiding massive increases in the death rate from starvation are dim indeed.

To the contrary, I have heard Roger Revelle (who is not the most enthusiastic booster) assert that it would not be difficult, with the new strains of rice and wheat, for Indian farmers in the areas where the land and water conditions are suitable to produce enough grain at the end of the century to support the population that India would have even with no reduction in human fertility at substantially higher levels of nutrition than today. (Of course I do not intend by citing this example to imply that India or other less developed countries can afford a long continuation of rapid population growth.)

In the reviewer's opinion this book would be better balanced and more persuasive if the authors had not chosen to ignore the very substantial body of work by social scientists on the problems they address. It is remarkable that a long book on the effects of overpopulation full of colorful historical allusions should contain not a single reference to T. R. Malthus, whose *Essay on Population* not only attracted the attention of the intellectual world in the early 19th century to the problem of excessive numbers, but had an effect (acknowledged by Darwin and Wallace) on the development of the theory of evolution. There is no recognition in a full chapter devoted to "optimum population and human biology" of the dozens of books and articles written on optimum population from the middle of the 19th century until the 1950's. Rather, the authors state: "The idea of controlling the size of the human population is really a new one."

The failure to cite others' work is not very important, but the failure to recognize the pertinence of the analysis used by social scientists is. An example is the neglect of the effect of cost and price on decisions. The Ehrlichs think in terms of fixed coefficients. Thus ". . . to raise all of the 3.6 billion people of the world of 1970 to the American standard of living would require the extraction of almost 30 billion tons of iron . . . ." But what the world would choose if incomes everywhere were greatly increased is not necessarily—in fact certainly would not be—the American bill of goods. The list of things we consume is partly influenced by relative prices, including how cheap raw materials are (because, it must be noted, they are still so abundant). It costs little extra in labor, and, since the steel involved is not expensive, it costs little more altogether to produce a big, heavy car than to produce a small, light one. If incomes increased dramatically in all the world (and no rate of increase in per capita income yet experienced would bring the income of all parts of the world to the current level in the United States by the end of the century), the dominant mode of transportation might well not be automobiles (a major user of steel), and if the cost of metal rose automobiles would contain much less of it. A book of this sort written in 1900 might have dismissed as impossible a U.S. population in 1970 of more than 200 million with a per capita income (estimated at constant prices) several times greater than in 1900, on the grounds that pasturage could not be found for enough horses.

The economist is trained to think in terms of substitutions that occur in response to changes in relative prices, and there is ample experience showing that the economy does respond to relative price changes by shifting from one product or material to another. A realization of this fact puts resource exhaustion in a different light. Social scientists are accustomed to thinking of alternatives that are marginally different, and one alternative is typically better in some ways and worse in others. In their proposals the Ehrlichs, hyper-aware of balances and interconnections in ecosystems, seem only occasionally aware of balances and interconnections in social systems. Their recommendations, applied literally, would cause massive unemployment and other forms of social disruption just as serious as the problems they discuss.

Doubtless social scientists (among others) have failed to appreciate the ecological implications of social change, and we are indebted to the Ehrlichs for forcefully bringing these problems to the attention of the world. But in acknowledging with gratitude their role in attracting our attention to the problems, we are not obliged to accept their explanation of how the problems arose, or their prescription for how they should be solved.

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Pipes and Tablets

The Davenport Conspiracy. MARSHALL MCKUSICK. University of Iowa Press, Iowa City, 1970. xii, 144 pp., illus. Cloth, $5; paper, $3.

In this provocatively titled work, the author has probed one of the unswept corners of Midwestern archeology and come up with an absorbing tale of 19th-

century skulduggery that masqueraded for a time as science. It begins with the finding, around 1873–80, of three inscribed stone tablets and two carved "elephant" pipes in Indian mounds in eastern Iowa. Directly involved were a Swiss-born clergyman with an irresistible urge to open mounds, several members of his family, and his sponsor, the Davenport Academy of Natural Sciences, one of the better and more productive of the many groups of locally organized amateurs who were experimenting in natural science in the decades following the Civil War.

The tablets were widely accepted at first as bearing on the origin and early history of the pre-Indian mound-builders; and translations were made of the inscriptions. The pipes were regarded as evidence of contemporaneity of man and mastodon, still a revolutionary idea at that time. Shipped to the Smithsonian, the tablets were exhibited informally to members of the National Academy of Sciences, where they reportedly aroused some interest but produced no recorded expressions of professional acceptance. Their authenticity was almost immediately challenged by an assistant in ethnology at the Smithsonian, whose unpublished report to the Davenport Academy was promptly repudiated by the Institution's Secretary. Upon the appearance of the elephant pipes, such well-known figures as John Wesley Powell, Cyrus Thomas, and Henry Henshaw of the Bureau of Ethnology, then deeply involved in studying the relationships of the mounds to the historic Indian tribes, came out strongly against their authenticity. Henshaw, most outspoken of the three, was vigorously attacked by American and some European protagonists of the finds.

Some Davenport Academy members early expressed suspicions, and made charges that spurious artifacts were being produced in the basement of the Academy building, allegedly as a hoax directed against the energetic preacher who was turning them up in the mounds. The president and principal benefactor of the Academy, innocent of the scheme, devoted his personal means and legal talent to the expulsion of disbelievers from the body and explored the possibility of lawsuits for libel against certain outsiders. By the late 1880's, the objects had been pretty generally rejected by nearly all except the faithful few in the declining Academy membership.

To archeologists, the main outlines of the Davenport episode have been known for many years. The present volume adds to the record much previously unpublished documentary material regarding the making and planting of the bogus artifacts, together with technical details on these and other specimens in the existing museum collections in Davenport. It all adds up to a sordid business and an unfortunate chapter in American archeology. Withal it is an interesting story, here well told.

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Scientists' Correspondence

Partners in Science. Letters of James Watt and Joseph Black. ERIC ROBINSON and DOUGLAS MCKIE, Eds. Harvard University Press, Cambridge, Mass., 1969. xvi, 504 pp. + plates. $12.50.

Shortly after the death of Joseph Black in 1799, the letters written to him by James Watt over a period of some 30 years were returned to Watt, who preserved them, with Black's answers, among his personal papers. This is the correspondence (approximately 160 letters) which forms the bulk of the collection published here. To it has been added every other Watt-Black letter the editors could find, all of the letters found exchanged by Watt and John Robison (over 40), a few related letters to or from other persons, and a transcription of an 83-page laboratory notebook kept by James Watt and referring primarily to experiments on the latent heat of condensation of steam. Many of the letters (we are not told the number) have never previously been published or have appeared only in extract.

As a set of primary documents these letters should prove useful to any student of the period or of the personalities involved. No doubt Robinson will use them in his subsequent studies of James Watt, but this is the last of the publications of the second editor, the late Douglas McKie, and it is particularly to be regretted that we are not, after all, to have the benefit of his years of study of Joseph Black, for the volume has been left bare of commentary and, except for the confusion of the laboratory notebook, annotations have essentially been restricted to the identification of persons or books mentioned in the correspondence.

The text of the correspondence seems meticulously edited. As far as they go, the annotations are moderately helpful, though Wedgwood's experiments, referred to in letter 98, are not those on his pyrometer but another set explicitly testing the Lavoisier-Laplace crushed-ice calorimeter, and it is surely more pertinent to know George Gleig as an editor of *Encyclopaedia Britannica* than as Bishop of Brechin.

Unfortunately, the annotations are not enough; the letters do not stand independently as a record of a partnership in science. The correspondence does not commence until 1768, at least a decade after Black had made the last of his major contributions to science—the discovery of latent and specific heats—and several years after Watt had completed his invention of the separate condensor. The references to these events are, therefore, historical recapitulations, and what remains is an uncharted account of comparatively minor year-to-year activities with, in later years, detailed accounts of the ailments of the correspondents. Yet there are items here which a commentary might have made significant to the ordinary reader. Letters to Black and J. H. Magellan in 1780, for example, not only reveal Watt's persistent effort to obtain for Black credit for his crucial work on heat, but also reveal two little-known published accounts of that work—both prior to 1772. Watt's comments to Black on Priestley's and his chemical experiments provide some interesting and useful sidelights on the discovery of the composition of water and on the theory of phlogiston, while the correspondence between Robison and Watt respecting the posthumous publication of Black's chemical lectures (much of which has previously been published and discussed by McKie in *Annals of Science*) casts renewed doubt on the integrity of that edition as a reflection of Black's ideas.

What might, by a detailed commentary, have been made an extensive review of the working relationship of two significant men must, instead, be supplemented by reading into the extant biographies of Watt, Black, and Robison. This volume is, therefore, essentially raw material of biographical revision. But for that we should be grateful. Rarely is there made available such a near-complete record of the friendship of two such important figures of science and technology.

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